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ABSTRACT

This paper has two major purposes: first, to consider how infant feeding behavior may fit into attachment theory; and second, to cite some evidence to show how an infant's early interaction with his mother in the feeding situation is related to subsequent development. It was found that sucking and rooting are precursor attachment behaviors that may become tied into the attachment system if a baby is allowed much initiative in attaining the breast, but that the behaviors do not necessarily become so. From an analysis of mother-infant interaction, it was concluded that the feeding situation does have a significant relationship to the kind of attachment the child develops, to later problems in feeding, and even to later so-called 'oral' problems that are not directly related to feeding. One of the conclusions of the study was that the time has come to reopen the issue of the effects of breast versus bottle feeding on both maternal behavior and infant behavior and the interaction between them. (CS)

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INFANT FEEDING AND ATTACHMENT

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This paper has two major purposes: first, to consider how infant feeding behavior may fit into attachment theory, and second, to cite some evidence to show how an infant's early interaction with his mother in the feeding situation is related to subsequent development.

Let us first consider the place of feeding behavior in attachment theory, that is, what role is assigned to feeding in the development of a young child's tie to his mother. In order to do this, we must first briefly remind you of the theory of the origins of this tie that attachment theory was proposed to replace. For a long time it was believed that the drive gratifications a baby received from his mother, especially in regard to feeding, were the essential source of his relationship with her--and the relationship was conceived as dependency. For the Hullians the relationship rested on a dependency drive secondary to the primary drive of hunger. For the Freudians, the relationship was defined as anaclitic--leaning on gratification of instinctual drives--especially oral drives--and from this anaclitic, dependency relationship a true object relation gradually emerged. This, too, is a version of secondary-drive theory. Three lines of evidence undermined this secondary-drive account of the origin of the infant mother relationship--imprinting, Harlow's work (e.g., 1958, 1961), and two longitudinal studies of the development of early attachments in humans--Schaffer and Emerson's (1964) and mine (Ainsworth, 1967).

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Meanwhile, Bowlby (1958, 1969) proposed a new approach to understanding the origins of a child's tie to his mother, an approach rooted in evolutionary

theory. According to him, the human species like many other species evolved certain behaviors identified as attachment behaviors, which served to promote proximity between an infant and his mother, this proximity serving the function of protection. An infant's attachment to his mother is built up through these behaviors--it does not rest on anything more primary.

An infant's repertoire of attachment behaviors includes signalling behaviors such as crying and smiling that draw others into proximity to him and more active behaviors such as grasping, clinging, reaching and approaching through which he himself may gain and/or maintain proximity or contact. Among these active contact promoting and maintaining behaviors--and perhaps the most efficient of them in the earliest weeks--are rooting and sucking. Obviously these behaviors play a dual role. They not only promote proximity and contact, but they are also part of the feeding system.

Bowlby (1969) has tended to minimize the role of sucking and rooting as attachment behaviors. There are a number of reasons for this, we believe, of which we shall mention only three. (1) Imprinting studies have shown that species that have locomotion soon after birth--some birds but also some mammals rapidly become attached to a figure or object that has nothing whatsoever to do with feeding but which is merely conspiciously present. Harlow (e.g., 1961) has shown that infant rhesus monkeys became attached to non-feeding terry-cloth mother surrogates rather than to feeding surrogates made of wire. It is the terry-cloth surrogate to which the infant clings, which provides a haven of safety when danger threatens, and a secure base for exploring a new situation. Furthermore, both Schaffer and Emerson (1964) and Ainsworth (1967) have shown that human infants can and do become attached to figures who have had nothing whatsoever to do with feeding them or with the gratification of any of the drives that have traditionally been considered to be primary. (2) In Western

society, with its widespread use of bottle feeding and feeding schedules, sucking and rooting do not seem to play an important role as attachment behaviors--and we will return to this later on. (3) Finally, I believe that Bowlby may have tended to soft-pedal the significance of sucking and rooting as attachment behaviors at least partly in reaction against the overwhelming overemphasis that has been given to orality by his fellow psychoanalysts.

As for feeding, Bowlby acknowledged that it was one of the predictable outcomes of attachment behavior in a mammalian species, but he gave it second place to another predictable outcome--protection. It was because an infant was protected when he acted so as to promote proximity to his mother that attachment behaviors were selected in the course of evolution. It also often turned out that he was fed. And indeed one can certainly argue that feeding is just as important for survival as protection--although feeding can be delayed whereas when danger threatens protection is more urgent.

In Harlow's laboratory the infant rhesus could choose between a terry-cloth surrogate to which he could cling and an uncomfortable wire surrogate with a nipple that yielded milk at scheduled intervals. In the natural environment, however, the rhesus does not have to make such a choice. He, and indeed all other species of monkeys and baboons that have been adequately observed, clings to his mother not only with arms and legs and hands and feet, but he also clings to her nipple with his mouth. Not all of the time that he clings thus is he actually sucking or receiving milk. Much of the time, apparently, he is merely holding the nipple in his mouth, and at least some of his sucking is shallow, non-nutritive sucking. Nevertheless, infants of such species are characterized as continuous feeders, even though it would be more accurate to say that they are frequent intermittent feeders who can initiate feeding at almost any time through their own rooting and sucking behavior.

The human neonate is obviously different. Although he can grasp reflexly with his hands, he cannot grasp with his feet, and he cannot on his own account maintain contact with his mother. Unless his mother were willing continuously to use one arm to support him at the breast, he could not hold the nipple continuously in his mouth. A newborn chimpanzee or gorilla, like a human neonate, is unable to maintain contact with his mother on his own account. His mother, however, does hold him most of the time in a ventro-ventral position, and even though he does not hold the nipple in his mouth consistently, he can find it fairly easily when he wants it, and he too is classified as a continuous feeder.

Is the human infant, like other primates, preadapted to be a "continuous" feeder, carried about by his mother wherever she goes and with easy access to the breast nearly any time he seeks it? Not many species are so preadapted. Some are precocial, and follow the mother wherever she goes, rather than being carried--and the human neonate obviously isn't one of these. In many mammalian species the mother caches her infants in some safe place while she goes about her other activities, and returns only at infrequent intervals to be with her babies and to feed them. Blurton Jones (1971) asked the question of whether the human species is, like other primates, basically a carrying species or a caching species--in a paper that he calls his cache or carry paper. Two of the several lines of evidence he cites are the composition of mother's milk and infant rate of sucking. The composition of human milk is relatively low in protein and fat, like that of carrying species whose infants are so-called continuous feeders rather than high in protein and fat like that of caching species whose infants are fed at infrequent intervals--like rabbits or rodents. The rate of sucking of the human infant, like that of

other primates, is much slower than that of the infants of caching species who suck at a very fast rate when indeed after long periods of absence the mother returns to feed them. Blurton Jones concluded that we are a carrying species in terms of our evolutionary adaptations, and thus the behavioral and physiological equipment of the human infant suits him to be a continuous or at least a frequent feeder rather than an infrequent feeder.

Now, in contemporary Western society most mothers do not behave as though they belonged to a carrying species, nor do they permit their infants to be frequent feeders. They cache their babies in a safe place, go about their other activities, and return to them from time to time. They feed their babies at four-hourly intervals, and often enough bend every effort to extend the intervals as soon as possible so that the baby is fed but three times a day. Bottle-fed babies receive a richer formula than mother's milk, and very early in life receive supplementary foods--not always just to assure good nutrition but also to fill him so full that he will not become hungry again for a long time. If a mother feeds by breast, she is likely to do so for only a few months. If she adheres to a feeding schedule, feeding comes not in response to infant signals or even active behavioral efforts, but at the mother's timing.

But how was it with the first humans--the hunting and gethering people who lived in the environment whose pressures selected the behaviors that were to be characteristic of the species? We can only speculate about them, but there is some recent evidence from present-day hunting and gathering societies such as the African Bushmen--the San--described by Konner (1971). San mothers do in fact carry their babies whenever they move about, and they use a sling adjusted in such a way that the baby's weight rests on the mother's hip, the

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baby is in a semi-ventro-ventral position to one side, so that he has easy access to the breast. And when these mothers are not actually moving about they tend to hold their babies. Judging from this line of evidence, also, humans seem to be preadapted to be a carrying species and to feed their babies frequently.

Let us turn to another African society with which I am myself familiar--the Ganda. I observed (1967) that Ganda mothers hold and/or carry their babies much more than we do, and rarely cache the baby away by himself as we so often do, although they do less carrying than do the San. But it is feeding that we want to talk about more than carrying. Among the Ganda families that I visited some fed the baby very frequently, giving him the breast whenever the slightest signal indicated that he might want it, and furthermore gave the breast for comfort if he seemed distressed for any reason even though he was not believed to be hungry. Some mothers fed the baby on a four-hour schedule. Some were intermediate--paying no attention to the clock; feeding the baby when he was judged to be hungry, but not merely for comfort. These differences pertained mainly to daytime feedings, however, for at night, the mothers reported, the babies were fed whenever they woke. Some babies slept with their mothers and thus had access to the breast, although more merely slept in the mother's room. Weaning was later among the Ganda than in our society among breast-fed babies, at about 12 months--although it was not as late as among Konner's San, or among the Zulu reported by Albino and Thompson (1956). Indeed weaning among the Ganda was traditionally much later, in the third or even fourth year of life.

Two observations of feeding interaction among the Ganda are especially pertinent to our discussion here. First, it was striking that those babies

who were fed on demand rather than on schedule--and especially those who were given the breast for comfort--soon became very active in their search for the breast. They approached the mother, clambered up on her lap (which was easy because she sitting on the floor) and fumbled in her clothing for the breast. The older ones managed to free the breast, grasp it, and suckle--managing the whole thing themselves. Those who were fed on schedule did not take this kind of initiative. And it was less likely that babies took such initiative among those who were fed on what resembles our kind of demand feeding, which really amounts to feeding the baby only when he is judged to be hungry.

Secondly, the response of Ganda babies to weaning is of considerable interest. Weaning among the Ganda takes place in two stages, both of which are preceded by a period beginning in early infancy in which increasing amounts of solid foods are introduced into the diet. The first stage of weaning extends usually over a period of two to four weeks, during which daytime breastfeedings are gradually dropped out. Most babies adapt to this with little disturbance--for a day or two at most. The second stage of weaning is abrupt. All the nighttime feedings drop out at the same time. The babies' response to this second stage was of particular interest. Unfortunately for our purposes here, only 10 of the 25 breast-fed babies in our sample were weaned during our period of observation, so the rest that we have to say must be viewed as hypothesis rather than conclusion. It was the babies who had been fed for comfort and who had been so active in initiating feeding on their own account who tended to be disturbed in this second stage of weaning. For them, weaning was as great a trauma as traditional psychoanalysts have always claimed weaning to be. Five of the weaned babies

had been fed on demand. One of these was not weaned until he was 21 months of age, and he showed relatively little disturbance, but the other four were disturbed for weeks afterward; indeed none of them had resumed normal relations with their mothers by the time the field study ended. They behaved like children after a traumatic institutional separation. They were anxious, cried easily, followed the mother wherever she went, and sought much more physical contact than before; whenever the mother got out of sight they were distressed, and they tended to resist care from substitute figures, even figures that they had readily accepted prior to weaning. The five babies who were weaned after having been fed on schedule, however, adapted themselves readily to the new regime.

Unfortunately this observation was based on but few cases, but I concluded, at least tentatively, that among those who had had easy access to the breast on their own initiative, both day and night, the feeding behaviors had become organized into the whole system of behaviors that constitute attachment, so that when the feeding relationship was destroyed by weaning, the whole attachment relationship was severely threatened and made very anxious. On the other hand, with those fed otherwise, the sucking and rooting and other behaviors that were involved in feeding became splintered off from the attachment system. They were addressed to food as an object and not to the mother as an attachment figure. When weaning came, it was a mere shift in mode of feeding which was minimally disturbing rather than a disruption of the whole organization of attachment behavior.

Please do not conclude that I believe demand feeding to be undesirable because it causes disturbance at weaning. My hunch, supported by one baby in my Ganda sample who was weaned much later than the others, at 22 months

of age, is that if weaning had taken place at two or three as it did traditionally among the Ganda it would not have caused disturbance. Furthermore, in our society even the most thoroughgoing demand feeding is very unlikely to give the baby the frequent access to the breast both day and night that the demand-fed Ganda babies enjoyed.

No, the major point I want to make is that sucking and rooting are indeed precursor attachment behaviors that <u>may</u> become tied into the attachment system if a baby is allowed much initiative in attaining the breast, but they do not <u>necessarily</u> become so tied in. They are not essential to support the development of attachment. We have a fail-safe system, as it were. There are enough other attachment behaviors to support the development of attachment. In our society it is probably very rare that feeding behaviors do become part of the attachment organization--but this was not always so.

On the other hand, we must not conclude that the kind of interaction that takes place between an infant and his mother is irrelevant to the kind of attachment relationship that develops, not even in our society. For the first two or three months of life feeding occupies a major proportion of the time that an infant is awake and in interaction with his mother. And this brings us to the second major part of this presentation in which we shall show that an infant's very early interaction with his mother in the feeding situation is related to subsequent development.

Our data here are drawn from 26 white, middle-class, American infant-mother pairs who were observed at home once every three weeks throughout the first year of life, each visit lasting about four hours. Nearly every visit spanned a feeding. This report today will focus on feeding in the first quarter and the last quarter of the first year.

Within this sample there was a wide variety of patterns of feeding. About half of the mothers said they fed on demand, but in fact no more than

Three even approximated demand feeding. The rest said they fed on schedule, but in six cases the schedule was so flexibly administered with such frequent between-meal snacks that it was difficult to distinguish from demand feeding. Five mothers, regardless of what they said, fed quite arbitrarily, with their interventions in no way geared either to a regular schedule or to the infant's signals. I can only conclude that mothers' reports on their feeding practices are so unreliable as to be useless; one must observe these practices directly.

After a thorough study of the feeding practices of this sample during the first quarter, which has been reported elsewhere (Ainsworth & Bell, 1969), four main dimensions were identified, in regard to each of which one could assess the degree to which a mother was sensitive to the baby's rhythms, signals, pacing and preferences, either gearing her behavior to his thus making him an active partner in the feeding interaction or herself being dominant in their transactions. These four dimensions were as follows: (1) the timing of feedings, whether in response to infant signals or not, (2) the determination of the amount of food ingested and the end of the feeding, (3) tact in handling of the baby's preferences when solid foods were introduced, and (4) the pacing of the rate of the baby's intake, whether in accordance with the baby's own rate or not. Four nine-point rating scales were devised, with each of five archor points given detailed behavioral definition. For our purposes here we shall consider only the mean scores derived from the four scales taken together, as a measure of the degree of maternal sensitivity-insensitivity to infant signals in regard to feeding. Mothers who scored as highly sensitive may be described as feeding "on demand" --if you like--but the scales provide a new and detailed definition of what constitutes demand feeding.

Our chief assessment of attachment was in terms of the baby's behavior in a laboratory strange situation at the end of the first year (Ainsworth, Bell & Stayton, 1971). This situation had seven main episodes--two pre-separation episodes followed by a separation and then a reunion episode, and then a second separation consisting of two episodes, followed by a final reunion episode. It was the behavior shown toward the mother in the two reunion episodes that was the chief basis of the assessment of attachment. Babies who are judged to be relatively secure in their attachment relationship to the mother showed heightened attachment behavior toward her in the reunion episodes, complicated by little ambivalent, resistant behavior and by little avoidant behavior. A second group avoided the mother in the reunion episodes, either ignoring her return or mingling attachment behavior with avoidant behavior. A third group did show heightened attachment behavior but mingled with resistant behavior, giving the impression of considerable ambivalence. I cannot cite the evidence here, but we have good reason to believe that both the avoidant and the ambivalent groups were anxious in their attachment relationship with the mother.

Now the securely attached group had mothers who on all the four dimensions of first-quarter behavior relevant to infant feeding showed greater sensitivity to infant rhythms, signals, preferences and pacings than did the mothers of the anxiously attached babies. The point-biserial r between the secure versus anxious dichotomy and the mean of the scales measuring the four dimensions for each infant-mother pair was .84 (p .0001), which shows a strong and highly significant relationship. It is clear that infants whose mothers have been sensitively responsive to them in connection with feeding in the first three months tend to develop a secure attachment relationship by the end of the first year.

This is not intended to imply that the mother's early feeding practices shape the quality of the attachment relationship to the exclusion of everything else the mother does in interaction with her baby. On the contrary, it must be emphasized that interaction with respect to feeding provides a very good sample of the total picture of mother-infant interaction in the first few months. Mothers who are sensitively responsive to infant signals in respect to feeding tend to be contingently responsive to many other aspects of infant behavior--promptly responsive to crying, well-paced in their responsiveness to vocalization, smiling and facial expressions generally in face-to-face situations, and also sensitive in physical contact interactions. Nor do we mean to imply that what happens in the first three months, in distinction to continuing interaction throughout the rest of the first year, finally determines the degree of security or anxiety of the attachment relationship. Mothers who are responsive to early infant feeding signals tend to continue to be responsive to signals throughout the rest of the first year. The mean scores of maternal behavior in the four dimensions of early feeding are highly correlated with ratings of general sensitivity-insensitivity to infant signals and communications in the fourth quarter ($r = .77$; $p < .001$). There is a continuity to interaction. Patterns of interaction that get established early on tend to persist. To be sure, there were some mothers who changed in the course of the first year--our correlations are not perfect. Nevertheless, our impression is that, for better or worse, experience in the first three months leaves its mark on the baby's behavior and affects the quality of the attachment relationship by the end of the first year.

Let us now turn from the relation of feeding to attachment to feeding itself, which, although perhaps not as important as the development of

harmonious social relations, certainly holds a place of some importance in its own right. One thing that impressed us from the analysis of feeding interaction in the first quarter was that the mothers who were rated low on our four scales for responsiveness to infant signals in feeding had babies whose first-quarter feedings could only be described as unhappy, because they were characterized by much crying, fussing, and/or tension. Needless to say these feedings were unhappy for the mother as well. (r_{pbi} = .61; p < .005). The unresponsive mothers also tended to have babies who spit up a lot--but babies who were over fed also spit up a lot, and some responsive mothers overfed their babies, and therefore the correlation was somewhat lower than for unhappiness (r_{pbi} = .46; p < .02).

An analysis of fourth quarter interaction relevant to feeding failed to discover dimensions of maternal behavior that seemed significantly related to infant-behavior. Sensitivity in timing of feedings did not seem important since infants were now on three meals a day and accepting this regimen. Mothers differed greatly in the degree of initiative and autonomy they permitted, for some babies were permitted to manage their own bottles or to feed themselves almost entirely, whereas other mothers continued to spoonfeed and to bottle feed. Some babies had been weaned from the bottle entirely, and accepted milk from a cup, but none could manage the cup alone unless they used a training cup with an inbuilt straw or spout. Some babies seemed content to be fed, others fiercely struggled for more autonomy in feeding, and some fussed or had temper tantrums for no apparent reason. To be sure maternal sensitivity-insensitivity to infant signals and communications in the fourth quarter was positively related to the degree of infant-unhappiness in feeding as assessed on a 7-point scale (r = .83; p < .001.). But we could perceive no clear relationship between <u>specific</u> maternal feeding practices

in the fourth quarter and the infant-mother attachment relationship, infant feeding behavior, feeding problems, or anything else.

But what was striking was that the degree of happiness these babies manifested in feedings between 9 and 12 months was strongly related to the mean score of their mothers on the four scales tapping maternal responsiveness to infant feeding signals during the first three months (r = .68; $p < .001$.) It seems that a baby's early feeding experiences carry over to influence the degree to which he enjoys feeding later on. Thus it seems very likely that feeding problems at the end of the first year of life and perhaps continuing on into the second year and beyond have their roots in the earliest months. We thought that spitting up that was persistent into the fourth quarter might be related to unhappy feedings, and indeed there was a significant relationship (r_{pbi} = -.46; p .02) between it and our fourth quarter happiness-unhappiness scores. But persistent spitting up in the fourth quarter was even more strongly related to maternal responsiveness to feeding signals in the first quarter (r_{pbi} = -.57; p .01.) Finally, and I must say to our surprise, we found that whether a baby did or not frequently suck his thumb in the fourth quarter was also negatively related to maternal responsiveness to feeding signals in the first quarter (r_{pbi} = -.50 p .01.)

In short, we believe that we have convincing evidence that the experience a baby has during his first three months in interaction with his mother in the feeding situation does have a significant relationship to the kind of attachment he develops, to later problems in feeding, and even to later so-called "oral" problems that are not directly related to feeding. This is the kind of evidence that a number of researchers in earlier

decades sought for in research into the effect of infant feeding practices. Caldwell (1964) reviewed this research and concluded that it yielded no evidence that it mattered whether a baby was fed on demand or by schedule, by bottle or breast, or when he was weaned. This part of her conclusions has been remembered, and indeed seemed to bring psychological research into infant feeding to an abrupt halt. But she also pointed out that these early feeding studies suffered from gross methodological defects, so that, in effect, the issues had not yet been subjected to adequate test. One of the most obvious defects was that information about maternal feeding practices was obtained invariably from interviewing the mother, often retrospectively at that, rather than by directly observing mother-infant interaction in the feeding situation. Certainly it appears that the whole issue of demand versus scheduled feeding should be reopened. Surely the essence of demand feeding is that the mother is responsive to infant signals, and, we suggest, this responsiveness is important not only in determing when feeding begins, but how it is paced and when it ends. Responsiveness thus broadly defined is shown by our evidence to be related to significant aspects of an infant's development.

If this is so in the case of the demand feeding issue, we may well wonder about the issue of breast versus bottle feeding. Unfortunately only four of our American sample were breast-fed and only three of these were assessed in our strange-situation procedure. These three, however, were all judged to be securely attached on the basis of this procedure, and the fourth, on the basis of other evidence much too complex to be summarized here, likewise seemed to be securely attached by the end of the first year. This, of course, can be merely suggestive. Rosenblatt (1965) suggests that

in the rat suckling, by causing the release of prolactin, has the two effects of promoting lactation and maintaining the maternal condition of the mother. Furthermore, he cites evidence that what he calls the "maternal condition" influences maternal behavior. It is plausible that in the human mother such hormonal support may, other things being equal, help her to be more sensitively responsive to her infant's feeding signals, and perhaps thereby more responsive to his other signals also. We believe that the time is ripe to reopen the issue of the effects of breast versus bottle feeding on both maternal behavior and infant behavior and the interaction between them. Short-term longitudinal studies involving intensive direct observation of mother-infant interaction are required.

Finally, as we implied earlier, further research into the time and method of weaning is clearly required, and also an examination of the interaction between infant feeding practices and time of weaning. For such research, a cross-cultural approach would be useful, because it would be difficult in our society to locate sizeable samples of mothers who do not wean their babies from the breast until the second and third year of life. And I say, wean from the breast. Previous studies of weaning have lumped together weaning from the breast and weaning from the bottle, whereas it seems very unlikely to us from the Ganda study (Ainsworth, 1967) that they are equivalent.

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